ROYAL ASTRONOMICAL SOCIETY.

VOL. XIII. *January* 14, 1853. No. 3.

J. C. ADAMS, Esq., President, in the chair.

Dr. Dick, Broughty Ferry, Dundee.
Capt. Ommanney, R.N.; and
G. V. Vernon, Esq., Manchester,

were balloted for and duly elected Fellows of the Society.

An Account of certain Experiments towards increasing facility and certainty in the Use of Mercury in Observations by Reflexion and for the Adjustments of Astronomical Instruments. By the Rev. C. Pritchard.

1. The difficulties and annoyances attending the use of mercury for instrumental adjustments are too well known by practical astronomers to require any description. In the present communication I propose to lay before the Society an account of certain experiments, which appear to disclose the source of these annoyances, and then to explain a method or methods by which I venture to believe they may be fully and with certainty and facility overcome.

2. My first attempt to modify or prevent the well-known tremors to which mercury is liable, consisted in placing a rim of amalgamated copper round the sides of the trough containing the mercury, and in roughening the bottom. I found this process diminished by half the quantity of mercury necessary to cover the bottom, but the tremors still continued, while the surface became dirtier than before.

3. The next expedient resorted to was the amalgamation of the *bottom* of the vessel, as well as that of the sides; thereby I found the quantity of mercury necessary to cover the bottom was about $\frac{1}{12}$th or $\frac{1}{15}$th of that required when the copper vessel was wholly unamalgamated. The surface of the mercury was also found to be exquisitely quiescent; the image of the wires in the telescope was decided and motionless, even when the meridian-shutters of the observatory were open, and the wind was blowing with sufficient force to extinguish the flame of gas: still the reflecting surface of the mercury was far from perfectly bright and clean. Attempts to remove the offending matter, either by skimming the surface or by

filtering the mercury through a cone of paper, failed to give a surface satisfactorily free from blemish.

4. Upon examining the mercury in the amalgamated copper vessel, after it had remained there untouched for twenty-four hours, and after gently inclining the vessel, I observed a phenomenon wholly unexpected. The entire surface was found to be covered with a somewhat tough film or pellicle, apparently consisting of long acicular crystals, starting, as it were, from the sides of the copper vessel, and chiefly from the lower parts of it, where the excess of mercury seemed to lift it up. If the vessel were carefully restored to its original horizontal position, the pellicle ceased to be visible: a small amount of violence fractured it, and then the line of fracture presented a permanent silvery line, reflecting but little light; upon further agitation the whole film broke up into minute particles, causing the surface of the mercury to assume somewhat of the appearance of frosted silver.

5. Upon pouring this mercury, thus contaminated, out of the shallow amalgamated vessel containing it, I found this film, or the greater portion of it, adhered so strongly to the bottom, that for a long time all my efforts to detach it were ineffectual. Wiping or gently scraping only served to divide it into smaller portions, and so, by floating on the top of any fresh mercury subsequently poured into the vessel, continued to injure the reflecting surface.

6. At length I devised a means of detaching this obnoxious film, by means of the percussion of a thin stream of mercury as it filtered through a cone of paper upon the bottom of the copper vessel held in a nearly vertical position. The film was thus gradually washed away and removed, and a small quantity of mercury, which was then suffered to remain in the vessel, presented the appearance of beautifully-faultless and highly-polished plate-glass. When this surface was placed under the telescope the reflexion of the wires was tremorless, black, and decided; so much so, that I was at first unable to distinguish the transit or the horizontal wires from their image.

7. This condition of the surface seemed to remain nearly unimpaired for five or six weeks, provided the vessel were permitted to remain undisturbed. Still, upon a closer examination, the existence of the pellicle or film was easily detected, and upon a slight agitation it broke up into the same small silvery portions I had before observed. Here, then, arose a formidable difficulty to the application of mercury thus treated to the observation of stars by reflexion, inasmuch as such observations require the repeated shifting of the reflecting surface.

8. In order to remove this obstacle, I tried the effect of amalgamation without the use of acid, conceiving that by some possibility there had been a formation of nitrate of mercury, inasmuch as the film under the microscope presented much of the appearance of this substance. But the film still made its appearance.

9. I then tried the effect of a vessel made of silver-electroplated copper. The adhesion of the mercury was perfect and in-

stantaneous, and the surface at first beautifully white, and perfect, and quiescent; but after a few hours the film, or a similar film, made its appearance just as before.

10. A copper vessel was then silvered by means of cyanide of potassium and oxide of silver: the surface of the mercury then became contaminated with a yellow film; but upon removing this by cyanide of potassium the reflecting surface was as perfect as in the former instances. Yet, after a few hours, the obnoxious film appeared as before.

11. Mercury placed in a tinned vessel presented a surface worse than any other observed.

12. It now became necessary to discover, if possible, what the pellicle was, and what was its origin. Hydrochloric acid was found to have no effect upon it; the film, therefore, did not consist of the oxide of a metal. Nitric acid entirely dissolved it, and upon applying the usual tests, the film appeared to consist entirely of metallic copper and metallic mercury, or of metallic silver and mercury, according as the vessel was either copper or silver. The mercury used was originally pure, but after standing in the shallow amalgamated vessels it was found to contain either silver or copper, but these metals only to a very slight extent, and in quantity by no means approaching the proportions in which they existed in the films. The source of these films now, therefore, appeared to be evidently the gradual floating up of the amalgams formed at the bottom of the vessels through the supernatant mercury. The existence, therefore, of these films seems to be an inherent and necessary consequence of amalgamation.

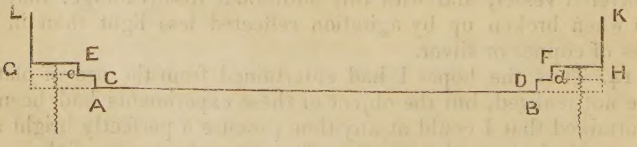
13. In this state of things I resolved to try the effect of a vessel made of platina. In order to effect the amalgamation of this metal, I in the first instance, adopted the very ingenious process pointed out by Mr. Grove, in an admirable paper printed in the *Philosophical Magazine for August* 1839, viz. that of making the platina dish, while in contact with mercury, the negative electrode in a voltaic circuit. Subsequently, however, I found that, provided the surface of the platina had been previously acted upon by nitro-muriatic acid, I could amalgamate the metal as perfectly and easily as I could copper, and that by simple friction only, without the intervention of the voltaic circuit. I had entertained the hope that the film of platina amalgam would now, on account of its density, remain at the bottom of the vessel, but it made its appearance at the surface of the mercury just as readily as in the case of a copper or silvered vessel, and with this additional disadvantage, that the film when broken up by agitation reflected less light than in the cases of copper or silver.

14. Thus the hopes I had entertained from the use of platina were not realized, but the object of these experiments had been so far attained that I could at any time procure a perfectly bright and comparatively tremorless surface of mercury, by means of the amalgamations and mercurial washing or percussion already described: this surface would remain effective for several weeks if undisturbed,

but did not, after a few hours, admit of unlimited removal from place to place.

15. Upon detailing these experiments to that very accurate philosopher, Professor Miller of Cambridge, he informed me that a paper upon the subject of the artificial horizon had appeared in Poggendorf's *Annalen*. On referring to p. 136 of the 79th volume, I found that M. Oertling, of Berlin, had there proposed an artificial horizon, constructed on the following principles:—1, a very shallow copper vessel was to be made, of a depth not exceeding $\frac{1}{120}$th of an inch; 2, this vessel was to be amalgamated by means of mercury and hydrochloric acid; 3, this exceedingly shallow vessel was to be placed upon a stand, supported by three adjusting foot-screws; 4, mercury was to be amalgamated to a very great extent with tin; 5, this amalgam of mercury and tin was to be poured into the shallow amalgamated copper vessel, and then the excess of mercury carefully skimmed off, leaving thereby a bright and tremorless surface. This surface, however, M. Oertling observes, is liable upon agitation, or from the access of wind, to become exceedingly foul. Of these five conditions, omitting or modifying four, and retaining only the fifth, viz. that of skimming off the surface, I now propose a construction of vessel, which I venture to regard as fulfilling all the requisites sought for in a perfect, and permanent, and movable reflecting surface. The surface itself is very easily obtained or renewed, and appears to be as applicable for the reflexion of stars as it is for the reflexion of the wires of a telescope. No adjusting-screws are necessary, no admixture of tin, no acid, nor any vessel of inconvenient shallowness, but such an one as may without difficulty be conveyed from place to place. The construction which I propose is as follows, admitting, however, such modifications as circumstances may require.

16. Upon a board of mahogany, or other wood, about $1\frac{1}{2}$ inch thick, is to be placed a perfectly flat rectangular plate of copper. On this copper is to be placed a flat rectangular rim, about $\frac{1}{20}$th or $\frac{1}{30}$th of an inch thick, and $\frac{1}{2}$ an inch wide. Thereby is formed a shallow rectangular vessel of copper, $\frac{1}{20}$th or $\frac{1}{30}$th of an inch deep. On three sides of this rim is to be placed a rim of iron or wood, $\frac{3}{8}$ths of an inch wide, and provided with a vertical flange $\frac{3}{8}$ths of an inch high; these three pieces of metal are to be screwed closely upon the mahogany. The section of the vessel thus formed is represented by the figure

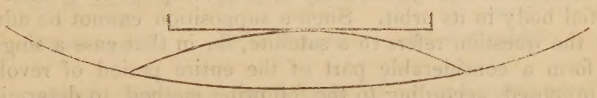


Where A B C D represents a section of the copper vessel, the depth A C = $\frac{1}{20}$th inch.

G E L is the iron or wooden angle-iron, forming a comparatively deep edge for the whole vessel.

The bottom A B, and the copper edges A C, D B, are to be carefully amalgamated by the friction of mercury with water and rotten stone, or pumice-stone, and then carefully washed with abundance of water, and dried by means of a piece of very clean linen. Pure mercury, free from lead or tin, is to be poured upon the bottom A B, and above the edges A C, B D. A flat piece of copper, with its edge amalgamated and cut so as to fit exactly the section G E C A D B F H, is to be drawn somewhat rapidly from the one end of the vessel to the other end, where there is no flange, so as to remove all the mercury, together with its floating impurities lying above the horizontal line C D, bodily into an iron box, placed in a suitable hole cut in the mahogany just under the extremity of the copper edge of the vessel. The whole is then to be tilted, until so much additional mercury has run out of the shallow vessel into the iron box as shall leave a depth of from $\frac{1}{80}$th to $\frac{1}{100}$th of an inch. The surface will be found to be perfectly quiescent, and free from all blemish, and may be carried with facility and without risk to any suitable position, and there placed upon a bed of sand. The sand I regard as very important, because it will at once facilitate the placing of the vessel in a horizontal position, and will also serve to destroy the propagation of tremors. It is evident, also, that the surface can, by means of mercury in the iron box, be renewed with the greatest facility in a very small space of time.

17. For ordinary purposes, it will be sufficient to form the vessel solely of flat copper turned up at the edges to the extent of about $\frac{1}{8}$th of an inch. This vessel is to be amalgamated according to the method directed, and then placed on the top of any vessel, such as a common saucer inverted in a large plate, as here represented :



Mercury can then be poured into the shallow amalgamated vessel, filling it above the brim ; the excess of mercury, together with its floating impurities, can then be skimmed bodily into the plate, and any additional quantity afterwards be tilted out until a sufficient quantity only remains in the vessel.

18. The quantity of mercury required for an amalgamated circular vessel, five inches in diameter, is under two ounces troy. If unamalgamated, the least possible amount is about twenty-one ounces.

19. The mercury after continued use will ultimately become very dirty and contaminated with copper, but I have found that the impurities may be wholly removed by the agitation of the impure mercury in a bottle containing a very little water. The impurities assume the form of a dark spongy mass, and while floating on the surface of the wet mercury can be with facility removed. The

mercury itself can then be dried with bibulous paper, and after being gently heated becomes pure and bright.

20. I will only add, that the modifications which these experiments suggest in the construction of the common portable artificial horizon are very obvious. With regard to the ingenious arrangements which, after the completion of these experiments, Colonel Sabine informed me had been adopted some years ago by M. Schumacher for the artificial horizon, I found the form of the vessel, viz. the segment of a sphere, peculiarly objectionable, on account of the depth and the quantity of mercury employed: moreover, there is no possibility of easily cleaning the surface, or of removing the obnoxious film of amalgam so often alluded to.

On the Determination of the Orbit which a Satellite describes around its Planet. By M. Sawitsch, Professor of Astronomy in the University of St. Petersburg.

When there exist a great number of observations of a satellite, we may employ various combinations for finding with facility the elements of the orbit which the revolving body describes around its primary. It appears, however, that no method has yet been assigned for determining these elements from the smallest possible number of observations. The general theory which we owe to the illustrious Gauss, relative to the calculation of planetary orbits, undoubtedly applies also to the problem under consideration. This theory first leads to an approximate solution; after which it puts us in the way of obtaining such a degree of accuracy as the excellence of the observations demands. The provisional results, however, necessarily imply that the intervals between the times of observation are small, relatively to the complete revolution of the celestial body in its orbit. Such a supposition cannot be admitted when the question refers to a satellite, for in that case a single day may form a considerable part of the entire period of revolution. It is proposed, according to the following method, to determine the elements from three observations, the interval between the first and third being supposed not to exceed a few days.

Let t, t', t'', denote the times of the first, second, and third observations; Δ, Δ', Δ'', the observed distances of the satellite from the planet; and Q, Q′, Q″, the angles of position referred to the circle of the geocentric latitude of the planet. Moreover, let l, l', l'', b, b', b'', denote the geocentric longitudes and latitudes of the planet: and R, R′, R″, its corresponding linear distances from the earth.

All these quantities are supposed to be given either by observation or calculation; but the following are to be determined, viz.:—

R $(1+\delta)$, R′ $(1+\delta')$, R″ $(1+\delta'')$, the linear distances of the satellite from the earth, corresponding to the times t, t', t''.

τ, the period of the revolution of the satellite.

ϱ, the angle under which the radius of the orbit of the satellite would appear at the mean distance of the planet from the sun.

i, the inclination of the orbit of the satellite to a plane passing through the centre of the planet parallel to the ecliptic.

ω, the longitude of the ascending node of the orbit measured upon the same plane.

ε, the longitude of the epoch.

Let the motion of the satellite, as well as that of its primary, be referred to a system of rectangular co-ordinates, the origin of which is supposed to be at the centre of the earth. Let us suppose that the axes of X and Y are in the plane of the ecliptic; also let the positive axis of X be directed to the longitude l', the positive axis of Y to the longitude $90° + l'$, and the positive axis of Z to the north pole of the ecliptic.

Let X, Y, Z, X′, Y′, Z′, X″, Y″, Z″, be the co-ordinates of the planet, and $X + x$, &c., $X' + x'$, &c., $Z'' + z''$, &c., those of the satellite corresponding to the times t, t', t''. Let dl, dl', dl'', represent the differences of geocentric longitude of the planet and satellite, and db, db', db'', the differences of latitude. Hence we have

$$\begin{aligned} X &= R\cos b\cos(l-l'), & X+x &= R(1+\delta)\cos(b+db)\cos(l-l'+dl)\\ Y &= R\cos b\sin(l-l'), & Y+y &= R(1+\delta)\cos(b+db)\sin(l-l'+dl)\\ Z &= R\,.\sin b, & Z+z &= R(1+\delta)\sin(b+db). \end{aligned}$$

Similar expressions may be obtained for the other co-ordinates of the planet and satellite.

We have also manifestly

$$dl = \frac{\Delta\,.\sin Q}{\cos b}; \qquad dl' = \frac{\Delta'\,.\sin Q'}{\cos b'}; \qquad dl'' = \frac{\Delta''\,.\sin Q''}{\cos b''};$$

$$db = \Delta\cos Q; \qquad db' = \Delta'\cos Q'; \qquad db'' = \Delta''\cos Q''.$$

Let us now transport the system of co-ordinates parallel to itself from the centre of the earth to the centre of the planet. The co-ordinates of the satellite relative to the centre of the planet will then be represented by $x, y, z, x', y', z', x'', y'', z''$. Remarking that $R\delta$, $R'\delta'$, $R''\delta''$, $\sin dl$, $\sin db$, $\sin dl'$, $\sin db'$, $\sin dl''$, $\sin db''$ may be considered as small quantities of the first order, the squares and products of which may be neglected; and bearing in mind further that $l - l'$, $l'' - l'$, do not amount, during an interval of several days, to more than a few minutes of space, we have without sensible error,

$$\left.\begin{aligned} x &= R\,.\,\delta\cos b - R\,.\sin b\,.\cos Q\,.\,\Delta\,.\sin 1''\\ y &= R\,.\sin Q\,.\,\Delta\,.\sin 1''\\ z &= R\,.\,\delta\sin b + R\cos b\,.\cos Q\,.\,\Delta\,.\sin 1'' \end{aligned}\right\}\ (A)$$

Similar expressions may be obtained for $x', y', z', x'', y'', z''$.

Let n denote the area of the triangle formed by the radii vectores of the satellite corresponding to the second and third observations, with the cord which unites their extremities. Let n', n'' represent the areas of the similar triangles corresponding to the first and third, and to the second and first observations. Since the satellite revolves in a plane passing through the centre of the planet, which coincides with the origin of the co-ordinates x, y, z, the following equations are deducible:—

$$n x - n' x' + n'' x'' = 0 \qquad (1)$$
$$n y - n' y' + n'' y'' = 0 \qquad (2)$$
$$n z - n' z' + n'' z'' = 0 \qquad (3)$$

Substituting in (2) the values of y, y', y'', as already determined, we have

$$\frac{n'}{n} = \frac{R \,.\, \Delta \,.\, \sin Q}{R' \,.\, \Delta' \,\sin Q'} + \frac{n''}{n'} \frac{R'' \,.\, \Delta'' \,.\, \sin Q''}{R' \,.\, \Delta' \,.\, \sin Q'} \qquad (\alpha)$$

Multiplying (1) by sin b', and (3) by cos b', and subtracting the one result from the other, bearing in mind at the same time, that as the geocentric latitudes of the principal planets are generally small, their alterations, $b - b'$, $b'' - b'$, in the space of a few days are inconsiderable, we have

$$\frac{n'}{n} = \frac{R \,.\, \Delta \,.\, \cos Q}{R' \,.\, \Delta' \,.\, \cos Q'} + \frac{n''}{n} \cdot \frac{R'' \,.\, \Delta'' \,.\, \cos Q''}{R' \,.\, \Delta' \,.\, \cos Q'} \qquad (\beta)$$

If we suppose the satellite to revolve in a circular orbit whose radius is r, we may employ the equations (α) and (β) in finding τ the period of revolution.

Let v'', v', v, be the angles described by the radius of the satellite during the intervals of time $t' - t$, $t'' - t$, $t'' - t'$. We have then

$$n = \frac{1}{2} r^2 \sin v, \; n' = \frac{1}{2} r^2 \sin v', \; n'' = \frac{1}{2} r^2 \sin v''$$

$$\frac{n'}{n} = \frac{\sin v'}{\sin v}, \qquad \frac{n''}{n} = \frac{\sin v''}{\sin v}$$

$$v = \frac{360^\circ}{\tau} (t'' - t'), \; v' = \frac{360^\circ}{\tau} (t'' - t), \; v = \frac{360^\circ}{\tau} (t' - t)$$

It is clear, then, that if an approximate value of τ was known, we might easily find v, v', v'', and hence also the relations $\frac{n'}{n}$, $\frac{n''}{n}$. Now it is always possible to obtain such a value of τ by successive trials. Assuming $\tau = 3$ or 6 or 10, &c. days, we calculate the corresponding values of $\frac{\sin v'}{\sin v}$, $\frac{\sin v''}{\sin v}$, and then we shall see which of the hypotheses satisfies best the equality of $\frac{\sin v'}{\sin v}$ and $\frac{n'}{n}$ as determined by (α) or (β). The corresponding value of τ will express the approximate value of the period of revolution. In order to avoid useless calculation, it will be desirable to take into consideration

the three angles of position Q, Q′, Q″, which will serve to show if the interval of time $t'' - t$ between the first and third observations approaches nearly or not to the time of a complete revolution. Knowing τ approximately, and repeating the calculation for two hypotheses of that element, differing slightly from each other, we may obtain a more accurate result, and in this manner we may approach nearer and nearer to the true value. When the angles Q, Q′, Q″, are very small, or very nearly equal to 180°, it will be advantageous to employ equation (β) instead of equation (α).

The following example, based upon three of M. Otto Struve's observations of *Neptune's* satellite, will serve to illustrate the practical utility of the method :—

Date.	M. T. Pulkowa.	Angular Distances.	Angle of Position with respect to the Circle of Latitude.
1847. Sept. 25	$10^h\ 21^m = t$	$15''{\cdot}92 = \Delta$	$228°\ 45' = Q$
28	$12^h\ 9^m = t'$	$17''{\cdot}30 = \Delta'$	$57°\ 42' = Q'$
30	$9^h\ 36^m = t''$	$5''{\cdot}07 = \Delta''$	$164°\ 51' = Q''$

Hence

$$t' - t = 3^d{\cdot}075,\ t'' - t' = 1^d{\cdot}894,\ t'' - t = 4^d{\cdot}969.$$

Moreover, the ephemeris of *Neptune* gives

$$\log R = 1{\cdot}46450,\ \log R' = 1{\cdot}46505,\ \log R'' = 1{\cdot}46540$$

It will be seen at once that the time of a complete revolution exceeds five days, for in 4·969 days the angle of position (Q″) has not nearly resumed its original value (Q). We may then commence by supposing $\tau = 5{\cdot}5$ and $\tau = 6$. The former of these hypotheses gives for $\frac{\sin v'}{\sin v}$ a numerical value by far *too small* in comparison with its equivalent $\frac{n'}{n}$ calculated from the formula (α). Taking $\tau = 6$, we obtain for $\frac{\sin v'}{\sin v}$ a value *too great*. If we now assume $\tau = 5{\cdot}7$ and $\tau = 5{\cdot}8$ we obtain

$$\log \frac{n'}{n} - \log \frac{\sin v'}{\sin v} = +0{\cdot}00643 \quad (\tau = 5{\cdot}7)$$

$$\log \frac{n'}{n} - \log \frac{\sin v'}{\sin v} = -0{\cdot}02390 \quad (\tau = 5{\cdot}8)$$

Thus the one hypothesis is 0·00643 in *excess*, and the other, 0·02390 in *defect*, and hence by proportional parts we find $\tau = 5{\cdot}72$. Repeating the calculation with $\tau = 5{\cdot}72$, we shall find that this hypothesis is erroneous only to the extent of + 0·00113, and we obtain definitively $\tau = 5{\cdot}724$ days. By a discussion of the totality of eighteen observations made in the years 1847–49, M. A. Struve found $\tau = 5{\cdot}876$ days, a result which accords sufficiently well with that deduced by an approximative process from three isolated observations.

When once we know the value of τ we may easily determine the radius of the orbit of the satellite.

Let r denote the linear value of this radius; A the semi-major axis of the orbit of the planet around which the satellite revolves; v'' the angle included between the two radii whose extremities are determined by the co-ordinates x, y, z, x', y', z'. We have then

$$\cos v'' = \frac{x x' + y y' + z z'}{r^2}$$

substituting for x, y, z, x', y', z' their values (A); bearing in mind also that $\varrho = \frac{r}{A \sin 1''}$ and that $r^2 = x^2 + y^2 + z^2 = x'^2 + y'^2 + z'^2$, we finally obtain

$$\varrho = \sqrt{\tfrac{1}{2} K^2 \pm \sqrt{\tfrac{1}{4} K^4 - M^2}}$$

in which

$$M^2 = \frac{\left(\frac{R}{A} \cdot \Delta\right)^2 \left(\frac{R'}{A} \cdot \Delta'\right) \cdot \sin^2 (Q' - Q)}{\sin^2 v''}$$

$$R^2 = \frac{\left(\frac{R}{A} \cdot \Delta\right)^2 + \left(\frac{R'}{A} \cdot \Delta'\right)^2 - 2 \left(\frac{R}{A} \cdot \Delta\right) \left(\frac{R'}{A} \cdot \Delta'\right) \cos (Q' - Q) \cos v''}{\sin^2 v''}$$

It is easy to select the proper sign of the quantity under the radical in the expression for ϱ, since the latter quantity is essentially greater than $\frac{R \cdot \Delta}{A}$, $\frac{R' \cdot \Delta'}{A}$, or $\frac{R'' \cdot \Delta''}{A}$. We may determine ϱ in a similar way by combining the third observation with the first or second. The most favourable combination is that in which $\sin v$ has the greatest value.

In the above example we have,

$$Q = 228^\circ 45' \qquad Q' = 57^\circ 42'$$

$$v'' = 193^\circ 22'; \qquad \frac{R}{A} \cdot \Delta = 15''{\cdot}445; \qquad \frac{R'}{A} \cdot \Delta' = 16''{\cdot}806$$

$$\tfrac{1}{2} K^2 = 206{\cdot}3; \qquad M^2 = 30624; \qquad \sqrt{\tfrac{1}{4} K^2 - M^2} = 109{\cdot}2$$

$$\varrho^2 = 315{\cdot}5; \qquad \varrho = 17''{\cdot}762$$

M. A. Struve has deduced $\varrho = 17''{\cdot}969$ from the totality of 18 observations.

We have now to calculate the planetocentric longitudes and latitudes of the satellite, $\lambda, \lambda', \lambda'', \theta, \theta', \theta''$, with respect to a plane passing through the centre of the planet parallel to the ecliptic. The expressions of the three planetocentric co-ordinates x, y, z, in which the direction of the axis of x is determined by the longitude l, lead to the following equations:—

$$\varrho \cdot \cos\theta \cdot \cos(\lambda - l) = \xi \cos b - \sin b \frac{R}{A} \cdot \Delta \cdot \cos Q \qquad (a)$$

$$\varrho \cdot \cos\theta \cdot \sin(\lambda - l) = \frac{R}{A} \cdot \Delta \cdot \sin Q \qquad (b)$$

$$\varrho \sin\theta = \xi \sin b + \cos b \cdot \frac{R}{A} \cdot \Delta \cdot \cos Q \qquad (c)$$

where $$\xi = \pm\sqrt{\varrho^2 - \left(\frac{R}{A} \cdot \Delta\right)^2}$$

Dividing (b) by (a) we find an expression for $\tan(\lambda - l)$, and as l or the geocentric longitude of the planet for the time t is known, we thus obtain the value of λ. The equation (c) serves to calculate θ, the planetocentric latitude of the satellite.

If we suppose the direction of the axis of x to be determined by the longitude l', and then by the longitude l'', we shall obtain equations analogous to (a), (b), (c), which will serve to calculate λ', θ', and λ'', θ'', by means of the data relative to the second and third observations.

When we have determined $\lambda, \theta, \lambda', \theta'$, the following formulæ will serve to calculate the inclination i, and the longitude Ω of the ascending node of the orbit of the satellite upon a plane passing through the centre of the planet parallel to the ecliptic.

$$\sin\left(\frac{\lambda' + \lambda}{2} - \Omega\right), \tan i = \tfrac{1}{2}(\tan\theta' + \tan\theta)\sec\tfrac{1}{2}(\lambda' - \lambda)$$

$$\cos\left(\frac{\lambda' + \lambda}{2} - \Omega\right), \tan i = \tfrac{1}{2}(\tan\theta' - \tan\theta)\operatorname{cosec}\tfrac{1}{2}(\lambda' - \lambda)$$

We might obtain i and Ω in a similar manner by combining λ'', θ'', with λ', θ', or with λ, θ.

In our example,

by combining λ', θ', with λ, θ, we have $\Omega = 264^\circ\ 46'$; $i = 37^\circ\ 35'$

— — λ'', θ'' — λ', θ' — $\Omega = 296\ 36$; $i = 30\ 28$

The difference of these two determinations, although sufficiently great, does not depend upon calculation. It arises from very small errors to which the best observations are liable, and which may exercise a very sensible influence according to the positions of the satellite with respect to the nodes at the times of observation. We may remark that the combination of the values $\lambda, \theta, \lambda', \theta'$, is not favourable for determining Ω, since the satellite was in those positions nearly -90° and $+90^\circ$ from the node. The combination of the second observation with the third is the most favourable for this purpose. As regards i both combinations are almost equally good. We admit, then,

$$\Omega = 296^\circ, i = 34^\circ$$

With these results, the equations

$$\tan u = \tan \cdot (\lambda - \Omega)\sec i;\ \Lambda = u + \Omega$$

will give the arguments of latitude u, u', u'', and the planetocentric longitudes Λ, Λ', Λ'', of the satellite in its own orbit.

In our example, $\Lambda = 201^\circ\ 34'$, $\Lambda' = 34^\circ\ 0'$, $\Lambda'' = 148^\circ\ 16'$. Designating by μ the mean daily motion of the satellite, we have—

$$\mu = \frac{\Lambda'' - \Lambda}{t'' - t} = 61^\circ{\cdot}72 \text{ by the interval of nearly 5 days.}$$

$$\mu = \frac{\Lambda' - \Lambda}{t' - t} = 62^\circ{\cdot}64 \quad — \quad — \quad 3 \ —$$

$$\mu = \frac{\Lambda'' - \Lambda'}{t'' - t'} = 60^\circ{\cdot}35 \quad — \quad — \quad 2 \ —$$

Whence the probable value of the mean daily motion,

$$\mu = 61^\circ{\cdot}72,$$

and the period of revolution,

$$\tau = \frac{360^\circ}{\mu} = 5{\cdot}832 \text{ days} = 5^d\ 19^h\ 10^m.$$

With $\mu = 61^\circ{\cdot}72$ we find the planetocentric longitude of the satellite upon its orbit, in 1847, September 28, at $12^h\ 9^m$ mean time to be—

By the first observation and the interval of	$3{\cdot}075^d$	.....	$31^\circ\ 14'$
second	—	—	0	.....	$34\ 0$
third	—	—	$1{\cdot}894$	.....	$31\ 26$
		Mean value	.....	$32\ 13$

The corresponding argument of latitude is $96^\circ\ 13'$. Subtracting the motion in 11·01 days, we have $136^\circ\ 45'$ for the argument of latitude on the 11th of September, at $11^h\ 55^m$ mean time. M. A. Struve determines the same argument, by means of eighteen observations, to be equal to $128^\circ\ 26'$. We are, therefore, in error only to the extent of $8^\circ\ 20'$.

As the inclination of the orbit of *Neptune* to the ecliptic is $= 1^\circ\ 47'$, and as the longitude of the ascending node of the orbit is $= 130^\circ\ 7'$, we hence deduce the longitude of the ascending node of the satellite upon the orbit of *Neptune* to be $= 296^\circ\ 37'$, and the inclination to be $= 35^\circ\ 44'$. We have now to explain the modes of verifying what has been already found, and to assign results of greater accuracy.

Since the trustworthiness of the preceding calculations depends upon the precise knowledge of τ the period of revolution, which has only been determined approximately in the first instance, it will be desirable to ascertain whether this value accords sufficiently well with the value $\tau = \frac{360^\circ}{\mu}$. The latter value ought to be in general the more accurate of the two, and if it differs considerably

from τ as determined by means of (α) or (β), it will be necessary to repeat the calculations.

Assuming $\mu = 61°{\cdot}72$, we have $\tau = 5{\cdot}832$ days. By the first calculations we obtained $\tau = 5{\cdot}724$. The difference does not amount to more than $0{\cdot}108$ days.

Let us now inquire into the degree of approximation of the angle ϱ as already determined. With $\mu = 61°{\cdot}72$ we have—

$$v'' = 189° 40'\ ;\ Q' - Q = 188° 58'\ ;\ \frac{R\,\Delta}{A} = 15''{\cdot}445\ ;\ \frac{R'\,\Delta'}{A} = 16''{\cdot}806\ ;$$

whence $\varrho = 20''{\cdot}40$

$$v = 116° 54'\ ;\ Q'' - Q' = 107° 8'\ ;\ \frac{R''\,\Delta''}{A} = 4{\cdot}929\ ;\ \frac{R'\,\Delta'}{A} = 16''{\cdot}806\ ;$$

whence $\varrho = 18{\cdot}27$

$$v' = 306° 42'\ ;\ Q'' - Q = 296° 54'\ ;\ \frac{R''\,\Delta''}{A} = 4{\cdot}929\ ;\ \frac{R\,\Delta}{A} = 15{\cdot}445\ ;$$

whence $\varrho = 18{\cdot}02$

As the errors Δ, Δ', Q, Q', produce upon the determination of ϱ an influence so much greater, as sin v'' is less, we may take—

$$\frac{20''{\cdot}40 \sin v'' + 18''{\cdot}27 \sin v + 18''{\cdot}02 \sin v'}{\sin v'' + \sin v + \sin v'} = 18''{\cdot}35,$$

for the probable value of ϱ, as deducible from three observations. A new calculation of Ω and i would diminish only in a small degree the uncertainty of these elements.

It remains for us to say a few words upon the mode of obtaining the elliptic elements of the satellite. Although four observations suffice *theoretically* for that purpose, still, considering the small value of the excentricity and the sensible influence of errors of observation, it will be impossible to attain the object by means of these data alone. Before undertaking the calculation of the elliptic elements it is desirable to ascertain the circular elements, and *that* to a certain degree of precision, so as to treat their corrections as small quantities of the same order of magnitude as the excentricities. It is necessary, besides, to have a long series of good observations, in order to deduce from them the elliptic elements of the orbit in a trustworthy manner.

The first thing to be done is to ascertain the degree of accuracy with which the observations are satisfied by the circular elements. For this purpose, it will be necessary to calculate with these elements the angle of position Q_0 and the apparent angular distance Δ_0 for the time of each observation. Representing by Δ, Q, the values of these quantities as deducible from the corresponding observations, we then form the differential quantities $\Delta - \Delta_0$, $Q - Q_0$, which will serve to exhibit the errors of the existing hypothesis respecting the orbit of the satellite. Now if the orbit be supposed to be in reality an ellipse, $\Delta - \Delta_0$ and $Q - Q_0$ may

generally be expressed in terms of the corrections of the circular elements and the excentricity and longitude of the perihelion. Let e be the excentricity and π the longitude of the peri-planet; then, putting $e \cos \pi = \psi$ and $e \sin \pi = \eta$, we may express $\Delta - \Delta_0$ and $Q - Q_0$ in terms of seven unknown small quantities, viz.: the corrections of the five elements already involved in the circular hypothesis and the quantities ψ and η. An equation of condition involving these quantities may be formed for every observation, whether of distance or position; and from the totality of such equations, the values of the unknown quantities may be deduced by the application of the method of least squares.

Extracts of a Letter from M. Otto Struve to the Astronomer Royal, dated $\frac{15}{3}$ January, 1853.

"As to what regards Villarceau's remarks on the differences in right ascension of feeble comets, as observed by different instruments and methods, I am quite of opinion that similar differences must exist, and that for different reasons, but principally produced by the different optical power of the telescopes. The nature of these differences is, perhaps, not yet sufficiently explained for all different methods of observation; but, generally speaking, the most powerful telescopes must furnish the most exact results; and for this reason, I think, Villarceau has done wrong to attribute a higher precision to the Königsberg observations than to those made with the Northumberland telescope, especially as Wichmann himself thinks the accuracy of his observations of D'Arrest's comet has seriously been injured by the faintness of the object, while Challis does not mention at all that these observations have given him particular trouble."

"Last month the University of Dorpat celebrated the fiftieth anniversary of its renewed existence. On this occasion I wrote a dissertation, entitled *Narratio de Parallaxi Stellæ α Lyræ*. In this tract I have given, besides some general remarks on the determination of parallaxes, the result of fifteen months' observation on the parallax of *α Lyræ*. The comparison star used by me is identical with that of my father; but the result does not so well agree with the former, as I thought it would do. My observations, which, as to accuracy, appear to be considerably superior to those of my father, make the parallax by the

$$\text{Angles of position} = +0''.145 \pm 0''.016$$
$$\text{By the distances} = +0''.140 \pm 0''.025$$

"In combining these results with the former values obtained by W. Struve and Peters, the parallax of *α Lyræ* is found in the mean $= +0''.1549 \pm 0''.0120$.

"I have indicated in the same tract several reasons, which will account in part for the larger parallax found by my father, but the

principal part of the difference must be attributed to an accumulation of accidental errors: since last summer I am engaged in a similar way with the parallax of 61 *Cygni*.

"The present winter is again very unfavourable for astronomical observations; since nearly six weeks we have not seen the sun. It is for this reason that I have not yet been able to publish my observations on Biela's comet, which require a new determination of the comparison star. The number of observations I have got of that object is very limited. They consist of five observations of the northern head, and three of the companion, made all in the interval between the 18th and 28th September, 1852; but I think they are all pretty exact, and will probably prove important, as it appears that this object has not been observed elsewhere, but by Prof. Secchi at Rome.* It was very interesting to see that the mutual action of the two heads still appeared to continue, notwithstanding their considerably increased relative distances. On the first day, Sept. 20, the southern head was larger than the other, and five days afterwards (Sept. 25) the northern head was brighter and considerably larger. It was obvious that the principal stream of light followed in both heads almost the direction of the line which joined the two heads.

"Mr. Lassell has sent me from Malta a circumstantial notice of his observations of *Saturn's* rings. Besides the transparency of the obscure ring, the most interesting observation appears to be the disappearance of the division in the obscure ring, which is confirmed by my own observations of the present winter. This division being easily seen last year, and with perfect surety, there is no doubt now, that the obscure ring undergoes rapid changes in very short intervals.

"It has given me great satisfaction to learn that the British Association has recommended to the Royal Society a new research on the irradiation of the Huyghenian telescope. The existing uncertainty in the amount of this quantity is, I think, the principal objection to be made against the results I have deduced. The result of the proposed research will, therefore, decide the question, which I could only exhibit in my memoir as the most probable result to be derived from the existing observations."

Extracts of a Letter from Sig. Secchi to Captain Manners, R.N. dated January 28, 1853.

"We have given instructions to Mr. Mertz to furnish us with a complete refractor, of 9 inches aperture and 14 feet focal length. The erection of the new observatory is going on well, and I hope before summer to have the principal part of the building itself quite

* At the time of writing this letter M. Struve was not aware that the comet had been observed at the Berlin Observatory (*Ast. Nach.* No. 832, p. 246), and also at Cambridge, by Professor Challis (*Ast. Nach.* No. 836, p. 325).—R. G.

ready. We shall have a revolving dome, 23 feet in diameter, for the equatoreal, and an elliptical room for the meridian circle, 20 feet long and 16 wide. In this a small transit instrument, 4 feet focal length, will be fixed as a collimator: and it will also have a strong cast-iron stand, to be used as a portable transit or prime vertical instrument. The Cauchoix refractor will be kept to make supplementary observations to the large equatoreal. A description of the new observatory will be published as soon as it will be in working order."

"Mr. Lassell has been kind enough to send me a very beautiful drawing of *Saturn*. Among the particulars which attracted his attention there is, as you know, the outline of the shadow, which is terminated by a prominent point at the place of the division of the rings; the appearance seemed to me a new proof that the rings are not plane; and in order to assure me of this fact experimentally, as far as was possible, I got made an artificial system like that of *Saturn*, having the surface of the ring slightly curved, as I imagine the real rings to be. The artificial rings have a curvature of about 1-20th of an inch, with a breadth of 3 inches. I exposed them to a very strong light of artificial lamps, and to the light of the sun itself, always taking care to have the incident rays inclined to the plane of the rings, according to their position in the heavens. Looking then through a telescope at this system, from an elevation like that of the earth, over the plane of the rings, I was struck with the perfect resemblance of appearances, as were several others, who witnessed the same. The curvature may be a good deal less than that used in my experiment, and still the phenomenon appears the same, as I deduced from another rough trial. I am, therefore, disposed to believe that the apparent curvature of the shadow of the ball over the ring, which turns the convexity towards the ball, is not a mere illusion of contrast of curvature but a real fact. I hope that, with the new instrument, something more satisfactory will be seen."

"The method of observing the sun with small diaphragms succeeds very well, and astronomical science will be very much indebted to Mr. Dawes for this simple contrivance. But to prevent the heating of the eye-piece, I have found that a diaphragm on single thick paper, covered with white lead, as used on visiting-cards, is perhaps the best. After some hours of observation the eye-piece is only slightly hot with our 6-inch object-glass refractor. Looking at the sun with this diaphragm on December 28, 1852, and January 1 of the present year, I observed some very interesting appearances in a very remarkable large spot. The most striking to me was that of a kind of very large ribbon, of almost oval shape, very like the edge of a lunar crater, but one part of which was projected like a bridge over a very black deep part of a nucleus. This appearance lasted only some hours, yet, during this interval of time, the outline of the crater and bridge was quite changed. I found some resemblance between these and the spots of the figures 6, 7, 10 of Sir John Herschel's observations at the Cape

of Good Hope. Another striking appearance was, that the edge of the inner penumbra was all like a saw, as if many streamlets of fluid matter were running towards the centre of the nucleus through several obstacles, or over a very rough bottom. The changes of this spot were very great, and I made several measures and drawings, but want of space prevents me telling you more. These appearances, however, seem to me very characteristic, and may throw great light on the physical constitution of the sun."

"The faculæ are, I think, fully proved to be prominences, by the fact that they are a good deal larger near the edge than at the centre of the disc. This I have observed several times during my experiments on solar heat. Some of these faculæ remained constantly visible during half a revolution of the sun, so that their identity was assured. I mention this merely as a confirmation of Mr. Dawes' discovery. I think the faculæ are brighter, only because their tops exceed the lower part of the solar atmosphere, which may absorb a good deal of light, since it absorbs at least nine-tenths of the total heat of the sun."

Accompanying the letter from which the foregoing passages have been extracted were a series of observations of *Massalia*, extending from November 18, 1852, to January 6, 1853, which were made by Signor Secchi at Rome, with the Cauchoix equatoreal. There were also two observations of Westphal's Comet, one made on the 5th, and the other on the 30th of December. The latter observation is interesting, as being considerably later than any hitherto published. The observations both of the planet and comet will, doubtless, appear in the *Astronomische Nachrichten*. In the meantime it may be useful to insert those relating to the comet. The following are the apparent positions:—

1852.	Rome M.T. h m s	App. R.A. m s	App. Decl. ′ ″
Dec. 5	7 41 58	*m* + 5 41·20	*m* + 13 36·3
5	7 55 5	*m* + 5 40·90	*m* + 13 37·8
30	7 39 32	*n* — 0 45·15	*n* — 10 55·2
30	7 45 57	*n* — 0 45·30	*n* — 10 52·8

The approximate places of the stars of comparison are—

	R.A. h m s	Decl. ′ ″
m	13 52 (20)	+ 66 4
n	13 11 (50)	+ 69 10

Letter from Mr. Lassell to Mr. De La Rue.

"I observe that your attention has continued to be drawn to the figure of *Saturn*, and that you have made a number of measures of his diameters; and also that Mr. Dawes has made some, though not under favourable circumstances. My drawings of *Saturn* did not profess to give the ellipticity in conformity with any measures

whatever, and I gave that form to the ball which satisfied my eye best on a comparison with the telescopic image. I have during my sojourn here made several sets of measures of the equatoreal and polar diameter, and the result is that $e = 0{\cdot}078764$ or $\frac{1}{12{\cdot}697}$ this compression (*apparent* compression — no correction being applied for present inclination of the axis) does not differ greatly from your own measures given in p. 21 of the *Notice*. Curiosity has led me to refer to my copy of the drawing sent last to the Society, and I find the compression is there graphically given *less* than the measurements show, viz. $0{\cdot}05485$, or $\frac{1}{18{\cdot}23}$ so that the drawing does not differ *so much* from the circle as either your own or my measures warrant.

" I presume that the 'something' which you thought you had discovered, but did not reveal in your note of the 20th November, is some inclination of the obscure ring to the plane of the others, evinced by an apparent encroachment of the dark ring on the bright one, and a want of symmetry in its elliptic projection. I can only remark upon this, that I have not observed such appearances ; and whether I might have been able in any degree to confirm your observation, if my attention had been especially directed to that point, I cannot now say. The weather has been lately unfavourable, even here, and *Saturn* is getting fast away now into the twilight.

" *Valetta, Feb.* 7, 1853."

Letter from the Rev. W. R. Dawes to Mr. De La Rue.

" Since I met you at the January meeting of the Astronomical Society, I have been looking over some observations of the diameters of *Saturn*, which I obtained in 1848. They are but few — only two sets of each diameter ; but the measures were taken under remarkably favourable circumstances. In my journal, I have noted that the planet was *exquisitely* defined, and all the measures excellent. As they were so few, and it was probable that the larger telescopes in some of the public and private observatories would be specially directed to the subject, I made no use of them, and was not aware of the ellipticity they indicated. On reducing them and calculating the result, I find from one of the sets, $e = \frac{1}{12{\cdot}163}$, and from the other, $e = \frac{1}{11{\cdot}813}$; the mean being $e = \frac{1}{11{\cdot}988}$, which is somewhat larger than I should have expected from my impression of the appearance of the planet at that time. Yet on drawing an ellipse of these proportions, it does not appear preposterous, as you will perceive in the diagram inclosed, along with one representing the ellipticity according to Mr. Main's observations. With respect to these, the Astronomer Royal has informed me that, at the time, such an ellipse was deemed *satis-*

factory to the eye when compared with the planet as seen in the telescope; so that I must withdraw my too hasty remark on that point, in a recent letter to yourself, which I perceive is inserted in the number of the *Monthly Notices* for November 1852. If I had had the opportunity of revising it, I should probably have modified the substance of the remark, and certainly the manner of it.*

"In the *Monthly Notices*, vol. x. p. 7, are some measures of the diameters of *Saturn* (10 of each) by the Rev. R. A. Thompson, with the Durham Fraunhofer equatoreal, which give $e = \frac{1}{11{\cdot}233}$. I remember being struck with the extraordinary compression shown in the diagrams of *Saturn* by Julius Schmidt, of the Bonn Observatory, contained in No. 650 of the *Astronomische Nachrichten*. On turning to them, I find they are evidently drawn to a scale, representing, probably, the results of observations; but *what* observations I do not know, as none are mentioned, I believe, in Schmidt's paper. The proportions of the diagrams give $e = \frac{1}{9{\cdot}667}$. It is to be regretted that the ellipticity assumed in the *Nautical Almanac* is not stated in the preface. Computed from the data for 1853, Sept. 1, it seems to be $e = \frac{1}{13{\cdot}035}$; but I am not aware from what observations this is deduced: and the result can be only approximate.

"*Wateringbury, Feb.* 12, 1853."

Sir William Herschel determined the ellipticity of *Saturn* to be $\frac{1}{10{\cdot}368}$ (*Phil. Trans.* 1806, p. 461), Bessel found it to be $\frac{1}{10{\cdot}199}$ (*Ast. Nach.* No. 275, p. 170). The following is a synoptic view of the different results:—

Main	0·10838	=	$\frac{1}{9{\cdot}227}$
Johnson	0·09940	=	$\frac{1}{10{\cdot}060}$
Bessel	0·09805	=	$\frac{1}{10{\cdot}199}$
Herschel I.	0·09630	=	$\frac{1}{10{\cdot}384}$
Thompson	0·08902	=	$\frac{1}{11{\cdot}233}$
Dawes	0·08341	=	$\frac{1}{11{\cdot}988}$
De la Rue	0·08139	=	$\frac{1}{12{\cdot}29}$
Lassell	0·07876	=	$\frac{1}{12{\cdot}697}$

* The letter from Mr. Dawes to Mr. De la Rue was a *private* one, and should have been referred to the writer *before printing*, but was not, either from hurry or inadvertence.—R. Sheepshanks.

It is to be borne in mind that only the first, third, and fourth of these results have reference to the *true* ellipticity of the planet. All the others represent merely the *apparent* ellipticity, as cited in the above letters of Messrs. Lassell and Dawes, and in a letter of Mr. Dawes, inserted in the *Notice* for November.

To obviate the flexure which is found to be so serious an objection in large reflectors, Lieut. Hardy proposes to cast the speculum with three or more bosses at the back, the bosses are to be fixed to a stout disc of cast-iron. It is thus expected that the flexure or distortion of the speculum while polishing or in use, may be resisted by the strength of the iron, which serves also for fixing and adjusting the speculum in its tube.

In compliance with a suggestion of the President, W. R. Grove, Esq., F.R.S., made a verbal communication of some means he had lately employed with success in improving ordinary refracting telescopes.

It is known, that in the object-glasses of these instruments the chromatic aberration is, as it is termed, over-corrected, while, practically, the spherical aberration is imperfectly or under-corrected. Mr. Grove had tried many expedients to remedy or abate these defects by some simple plan applicable to existing instruments.

If the glasses be separated a slight distance, as recommended by Sir J. Herschel, the chromatic aberration may be corrected, but the spherical aberration is generally increased to such an extent as to greatly deteriorate the performance of the instrument. In some cases where the inner curves of the flint and crown glasses approximate, Mr. Grove had employed with success a highly-refracting cement, made of very clear rosin and castor-oil, which, acting as a third lens or convex meniscus of a medium dispersing the coloured spaces differently from the other two lenses, corrected to a very great degree the chromatic without increasing the spherical aberration. This compound forms an excellent tough cement; it is nearly, and may be made absolutely, colourless, and might possibly be used in small telescopes instead of a flint-glass.

With telescopes in which the curves would not admit of a cement he had tried lenses of plate-glass, placed at opposite distances between the object and eye-glasses of the telescope; this plan he conceived was applicable to by far the greater number of common telescopes, and much improved their performance. It differs from the plans of Littrow, Rogers, or Barlow, in consisting of a *convex* lens, and in being applicable, at a very trifling expense, to telescopes constructed in the ordinary way. Mr. Grove had tried various curvatures and distances of this interposed lens, but as his experiments were made for amusement, and with no notion of publication, he had not noted the details. The following was the best result he had obtained :—

In a 5-foot 4-inch telescope, having a clear aperture of 3·6 inches, a small plane convex lens of plate-glass, of 6-feet focus, was placed, at a distance of one foot from the eye-glass, with the plane side towards the eye; the diameter of this lens need not be more than an inch, and it can generally be attached with ease to the inner extremity of the sliding eye-tube. This had produced so beneficial a result that a mediocre instrument had been changed into a very good one,—showing, for instance, the inner ring of *Saturn*, defining beautifully the division in the main ring, and dividing double stars well up to a second of space.

The effect of such a lens is three-fold. 1st, It brings back the over-correction of the object-glass; 2dly, being of a glass acting somewhat differently on the coloured spaces, it causes them to overlap, and gives a greater residuum of white light; and, 3dly, upon the principle of the Huyghenian eye-piece, it lessens the spherical aberration by receiving the more refrangible at a less oblique incidence than the less refrangible rays.

It will be obvious, that the nearer the interposed lens is to the object-glass the less should its curvature be, but in proportion as the object-glass is approached the difficulty of centering and defects in curvature become more felt; if, again, the interposed lens be brought very near the eye-glass, less effect upon the aberrations is produced.

Practically, lenses having foci somewhat greater than the object-glass, and placed at a distance from the eye-glass of from one-fourth to one-fifth of the focal length of the object-glass, will be found to answer; and the effect being one of degree, no mathematical accuracy in the amount of curvature is required.

WESTPHAL'S COMET (II. 1852).

This comet has finally disappeared from the observations astronomers. It was first discovered by Dr. Westphal, at Göttingen, on the 24th of July, 1852 (*Ast. Nach.* No. 819, p. 43). In all probability, the last observation of it was one which was made by Sig. Secchi, at Rome, on the 30th December following. A great number of observations of this interesting object will be found in Nos. 820–847 of the *Astronomische Nachrichten*. Elliptic elements have been calculated by Sonntag, which assign to it a period of 60·8 years (*Ast. Nach.* No. 836, p. 321). M. Marth, of the Königsberg Observatory, from observations separated by a wider interval, has deduced elements indicative of a period of 58·35 years (*Ast. Nach.* No. 839, p. 379). A more accurate set of elements will doubtless be obtained by a discussion of the totality of the observations. Some remarks on the physical appearance of the comet by M. Reslhuber of Kremsmünster, and M. Fearnley of Christiania, will be found in Nos. 846–7 of the *Astronomische Nachrichten*. The former of these observers, in a note relative to its appearance on the 26th of August, states that the tail had *a decided*

resemblance to a fan. On the 11th of October M. Fearnley perceived a short ray of light, about 1′ in length, issuing from the nucleus in the direction of the tail. It is worthy of remark, that this singular appearance was witnessed on the same night by Mr. Hind, at the observatory of Mr. Bishop, Regent's Park (*Ast. Nach.* No. 839, p. 371). It may be mentioned, also, that phenomena of the same nature were perceived by Kepler in the great comet of 1618 (*De Cometis*, p. 103), and by Sir John Herschel in Halley's Comet (*Ast. Obs. at the Cape of Good Hope*, p. 396). The comet at one time was distinctly visible to the naked eye. It was so seen by M. Reslhuber on the 7th of October. It ceased to be generally visible even in telescopes before the middle of December.* Only *one* observation of a later date exists, so far as is hitherto known, viz. the observation by Sig. Secchi already referred to.

Ascensions droites relatives des 36 Etoiles fundamentales, déduites des Observations faites à l'Observatoire Royal de Greenwich depuis 1750 jusqu'en 1762, et depuis 1836 jusqu'en 1850. (Second Partie.) Par M. U. J. Le Verrier.

(*Comptes Rendus, Dec. 6, 1852.*)

In the *Comptes Rendus* for the 5th of April, 1852, there is a brief account of the first part of this important memoir. In the December number M. Le Verrier gives the corrections which should be applied to the right ascension of each star (as computed in the *Tabulæ Regiomontanæ*) for every ten years from 1750 to 1850. These corrections are deduced by the aid of two Greenwich catalogues, one of which is founded on the observations of Bradley and the other on the modern observations extending from 1836 to 1850, executed under the superintendence of the present Astronomer Royal.

One of the most interesting results of M. Le Verrier's researches is the establishment of the *variability* of the proper motion of *Sirius*. This remarkable fact, the original discovery of which is due to Bessel, although its existence has been subsequently disputed, is clearly indicated by the observations of Bradley, and still more so by those upon which the catalogue of 1850 is founded. It appears, also, that the corrections to the place of the star agree both in quantity and direction with the hypothesis which supposes the star to revolve round a large opaque body. This hypothesis more especially accounts for the positions of *Sirius* from 1830 to 1850 with the most perfect accuracy. Indications of variability are also perceptible in the proper motion of *Procyon*, but the results in this case are not sufficiently unequivocal to justify a definitive conclusion on the subject. The following are the corrections above mentioned, which are to be applied according to their signs to the data of the *Tabulæ Regiomontanæ* :—

* M. Fearnley's observations of the comet come down to Dec. 6; M. Reslhuber's, to Dec. 10; and Mr. Hartnup's, of the Liverpool Observatory, to Dec. 12 (*Ast. Nach.* No. 840, p. 393).

Corrections of the relative Right Ascensions of the 36 Fundamental Stars comprised in the Tabulæ Regiomontanæ.

(Expressed in thousandths of a second of time.)

	1750	1760	1770	1780	1790	1800	1810	1820	1830	1840	1850
γ Pegasi.....	+108	+ 90	+ 72	+ 53	+ 34	+ 13	— 8	— 28	— 49	— 70	— 92
α Arietis.....	— 46	— 35	— 24	— 13	— 3	+ 8	+ 18	+ 27	+ 36	+ 44	+ 53
α Ceti	+ 11	+ 12	+ 13	+ 14	+ 15	+ 16	+ 17	+ 17	+ 17	+ 17	+ 17
Aldebaran ...	— 26	— 17	— 8	0	+ 9	+ 18	+ 28	+ 37	+ 46	+ 56	+ 66
Capella	— 24	— 23	— 21	— 19	— 15	— 9	— 1	+ 7	+ 16	+ 26	+ 37
Rigel	+ 51	+ 49	+ 47	+ 45	+ 43	+ 40	+ 38	+ 36	+ 33	+ 30	+ 27
β Tauri	+163	+142	+121	+101	+ 81	+ 62	+ 42	+ 23	+ 5	— 13	— 30
α Orionis	+ 42	+ 38	+ 33	+ 29	+ 25	+ 21	+ 17	+ 13	+ 10	+ 7	+ 3
Sirius										+263	+114
Castor	— 61	— 43	— 24	— 5	+ 14	+ 34	+ 54		+353	+373	+394
Procyon	— 58	— 57	— 55	— 53	— 50	— 46	— 42	— 37	— 31	— 26	— 20
Pollux	—112	— 94	— 75	— 56	— 37	— 18	+ 1	+ 20	+ 40	+ 61	+ 82
α Hydræ	—101	— 82	— 63	— 45	— 27	— 9	+ 9	+ 27	+ 44	+ 61	+ 78
Regulus	—122	—102	— 82	— 63	— 45	— 27	— 9	+ 9	+ 26	+ 43	+ 59
β Leonis	—106	— 87	— 68	— 50	— 32	— 15	+ 2	+ 18	+ 34	+ 49	+ 64
β Virginis ...	+ 9	+ 13	+ 18	+ 22	+ 27	+ 31	+ 35	+ 38	+ 40	+ 42	+ 44
α Virginis ...	— 19	— 19	— 19	— 19	— 19	— 19	— 20	— 21	— 23	— 25	— 28
Arcturus	— 34	— 23	— 13	— 4	+ 4	+ 11	+ 16	+ 20	+ 22	+ 23	+ 23
α^1 Libræ.....	+136	+120	+103	+ 85	+ 66	+ 48	+ 29	+ 9	— 10	— 30	— 49
α^2 Libræ.....	+228	+200	+172	+144	+115	+ 87	+ 58	+ 29	0	— 30	— 60
α Coronæ ...	— 32	— 25	— 17	— 9	— 1	+ 6	+ 11	+ 17	+ 24	+ 30	+ 36
α Serpentis ..	+165	+142	+118	+ 94	+ 70	+ 46	+ 21	— 3	— 28	— 53	— 78
Antares	+106	+ 89	+ 72	+ 56	+ 39	+ 22	+ 5	— 12	— 28	— 45	— 62
α Herculis ...	— 56	— 52	— 47	— 43	— 39	— 35	— 32	— 29	— 26	— 22	— 19
α Ophiuchi...	—141	—114	— 86	— 59	— 32	— 6	+ 20	+ 46	+ 71	+ 97	+122
Vega	— 52	— 46	— 40	— 33	— 27	— 21	— 15	— 9	— 3	+ 3	+ 8
γ Aquilæ	+176	+151	+126	+101	+ 75	+ 50	+ 24	— 2	— 28	— 54	— 81
α Aquilæ	— 12	— 12	— 13	— 13	— 14	— 15	— 16	— 17	— 18	— 18	— 18
β Aquilæ	+152	+128	+102	+ 75	+ 48	+ 21	— 7	— 35	— 64	— 93	—123
α^1 Capricorni	— 51	— 49	— 46	— 44	— 42	— 40	— 38	— 36	— 35	— 33	— 32
α^2 Capricorni	+ 20	+ 11	+ 3	— 6	— 15	— 25	— 35	— 44	— 54	— 64	— 74
α Cygni	—110	— 96	— 82	— 68	— 55	— 42	— 30	— 18	— 6	+ 6	+ 17
α Aquarii ...	— 97	— 87	— 77	— 68	— 59	— 50	— 41	— 33	— 25	— 17	— 10
Fomalhaut ...	— 58	— 69	— 81	— 94	—107	—121	—135	—149	—163	—177	—191
α Pegasi	+ 6	+ 7	+ 8	+ 8	+ 8	+ 8	+ 7	+ 5	+ 3	+ 2	0
α Andromedæ	— 13	— 4	+ 5	+ 14	+ 22	+ 30	+ 37	+ 44	+ 51	+ 57	+ 62

From 1750 to 1810 the correction for *Castor* refers to the centre of light of the two stars. From 1830 to 1850 it refers to the position of the *second* star.

Table of the Correction peculiar to the Star Sirius.

	s		s		s
1750	+0·219	1760	+0·005	1842	+0·286
1751	·185	1761	·001	1843	·279
1752	·153	1762	·000	1844	·261
1753	·123			1845	·236
1754	·096	1836	·162	1846	·210
1755	·073	1837	·187	1847	·184
1756	·053	1838	·213	1848	·159
1757	·036	1839	·239	1849	·136
1758	·023	1840	·263	1850	+0·114
1759	+0·013	1841	+0·280		

A curious anomaly offered itself to M. Le Verrier in the course of his researches on Bradley's observations. The differences of the mean right ascensions of stars distant about twelve hours from each other were found to be subject to a small periodic variation, which appeared to pass through the cycle of its values in the course of every twelve months. This variation amounted at its maximum to $0^s{\cdot}06$. M. Le Verrier seems disposed to attribute it to an imperfect compensation of the pendulum. This circumstance recalls to mind an idea formerly broached by Mr. Sheepshanks, viz. that of dispensing with all mechanical contrivances for compensation, by placing the pendulum at such a distance below the surface of the earth as to render it inacessible to rapid fluctuations of temperature. A similar mode of getting rid of the consequences of imperfect compensation has been recently proposed by M. Faye, who justly remarks, that the invention of the electric telegraph suggests a method of peculiar facility for reducing it to practice.

But it may be mentioned, that the modern Greenwich Observations have not offered to M. Le Verrier any trace of the anomaly above referred to.

METIS.

Professor Wolfers, of Berlin, has investigated new elements of this planet, which he has employed in calculating an ephemeris for the opposition of 1853. The ephemeris extends from Sept. 17 to Oct. 31 (*Ast. Nach.* No. 845, p. 75).

THETIS.

An ephemeris of this planet has been calculated by Dr. Hartwig, of Leipsic, for the opposition of 1853. It extends from April 16 to August 30 (*Ast. Nach.* No. 847, p. 103).

CALLIOPE.

An ephemeris for the opposition of 1853, extending from Feb. 1 to April 6, has been calculated by M. Hornstein, of the Vienna Observatory (*Ast. Nach.* No. 847, p. 111).

On the Variable Stars S Cancri and Algol. By Professor Argelander (*Ast. Nach.* No. 845).

M. Argelander has recently been devoting attention to the star *S Cancri*, which has been found to undergo periodical fluctuations of brightness analogous to those which distinguish the well-known variable star *Algol*. On the 19th of December, 1852, he perceived the star when its light was very faint. The interposition of clouds prevented him from observing the precise instant of minimum brightness, and when the sky again cleared up, the light of the star had already sensibly increased. He is of opinion, however, that the period of variation as ascertained by him, viz. $9^{d}\ 11^{h}\ 37^{m}$, cannot differ more than two minutes from the true value. All his observations of the star since February 16, 1850, agree very well with this determination; but he has not yet succeeded in observing an exact minimum. As the star affords a visible minimum only every nineteen days, there is little hope of a single individual accomplishing much by means of such observations, and it were therefore desirable that other astronomers should direct their attention to this remarkable object. A 5-foot Fraunhofer, or even a 4-foot instrument with a dark sky, will suffice for this purpose, and the star is easy to find by means of its situation with respect to *Præsepe* and *δ Cancri*. The following are the positions of the star, and also those of the stars of comparison for the mean equinoxes of 1800 and 1850.

		1800. R.A. h m s	1800. Decl. ° ′	1850. R.A. h m s	1850. Decl. ° ′
Var.	*S.*	8 32 30	+19 44	8 35 22	+19 34
8 to 8·9	*a*	34 20	+19 46	37 12	+19 36
9	*b*	31 40	+19 46	34 32	+19 36
8	*d*	30 44	+19 38	33 36	+19 28
10	*e*	34 15	+19 35	37 7	+19 25

The variable star, and also *a* and *b*, are to be found in Bessel's Zone, No. 277. Lalande has observed each of the stars *a* and *d* twice; *e* is estimated by the eye; it forms with *b* and *S* nearly a right-angled isosceles triangle, the right angle being at *S*. The visible minima for the month of March are,

March 5 ..... 7^{h}
— 24 6

M. Argelander recommends to those astronomers who may be desirous of aiding in such observations, to compare the variable star with either of the standard stars, by bringing them one after the other into the *centre of the field of view*, and not to observe both objects of comparison with the telescope in a fixed position, since the star which stands out from the centre always appears *too bright*. When it attains its minimum, *S* will be very nearly equal in brightness to *e*. M. Argelander remarks, that it begins to increase very rapidly; and he is inclined to suppose that its descent from the maximum is even still more rapid.

The following table of the minima of *Algol*, which are visible in Europe during the present year, has been calculated by M. Argelander.

Visible Minima of Algol in the Year 1853.

	Bonn M.T.								
	h m			h m			h m		
Jan. 6	20 20	May	21	14 38		Oct. 3	8 56		
9	17 9		24	11 27		6	5 45		
12	13 58		27	8 16		17	17 1		
15	10 46	June	10	16 20		20	13 50		
18	7 35		13	13 9		23	10 39		
21	4 24		16	9 58		26	7 28		
29	18 51	July	3	14 52		29	4 16		
Feb. 1	15 40		6	11 40		Nov. 6	18 43		
4	12 29		9	8 29		9	15 32		
7	9 18		23	16 34		12	12 21		
10	6 7		26	13 23		15	9 10		
21	17 22		29	10 12		18	5 59		
24	14 11	Aug.	1	7 1		26	20 25		
27	11 0		15	15 5		29	17 14		
March 2	7 49		18	11 54		Dec. 2	14 3		
16	15 53		21	8 43		5	10 52		
19	12 42	Sept.	4	16 47		8	7 41		
22	9 31		7	13 36		11	4 30		
April 8	14 25		10	10 25		19	18 57		
11	11 13		13	7 14		22	15 46		
14	8 2		24	18 30		25	12 34		
28	16 7		27	15 19		28	9 23		
May 1	12 56		30	12 7		31	6 12		
4	9 45								

Elements of Comet III. 1847, *deduced from the totality of the observations* (*Ast. Nach. No.* 845, p. 77).

This comet, which was discovered by M. Mauvais on the 4th of July, 1847, is remarkable for having continued visible till the month of April, 1848: M. Gautier, having calculated elliptic elements from three observations, found the time of a complete revolution to be 31,500 years. This result induced him to confine his attention to the investigation of such elements as would best represent the motion of the comet in any assigned part of its course. To this end he corrected the elements by means of the totality of the observations, taking into account the perturbations of *Jupiter*, *Venus*, and the earth; and then compared the results of this correction with eight normal positions of the comet, deduced from the observations. The agreement, except in one or two instances, is sufficiently satisfactory.

London:—Printed by G. Barclay, Castle St. Leicester Sq.